

Peer-review report of

Lawson, M.A., & Kakkar, H. (2024). Resolving conflicting findings in misinformation research: A methodological perspective. *advances.in/psychology*, 2, e235462.
<https://doi.org/10.56296/aip00031>

Round 1

Dear Authors,

Thank you for submitting your important work to our special issue. I was fortunate to receive reviews from two esteemed experts in the field, and I thank them for their detailed feedback. I also read and evaluated the paper myself before consulting their evaluations. We all find many strengths in your work. You focus on an unsettled debate and present comprehensive perspectives and nuances. The paper is also written in a quite engaging way. However, larger revisions are needed before the paper can be considered for publication. In addition to the points raised by the reviewers, please address the following points. Provide an overview of how you addressed each issue in the revision letter, including excerpts demonstrating the changes.

STRUCTURE

While the paper follows a narrative approach, I often struggled to understand the type of analysis conducted, and many details were missing. Therefore, I ask you to structure the paper in the standard format: introduction, methods, results, and discussion. Ensure all results are moved to the results section. Provide all details needed to replicate your analyses. Please include sections describing the samples, measures, and, most importantly, the analytical approach, which leads to my next, most major, comment.

ANALYSES

Your general argument seems to be that differences in results between the sets of studies are due to analytical differences. However, it is not entirely clear what causes this difference. Is it the higher power? Is it the choice of analyses? More clarity would be beneficial.

Most critically, although this may be due to the lack of details presented, I was not convinced by your analytic approach. Especially in a paper arguing that an interaction effect exists when data is appropriately analyzed, directly comparing different analytic approaches would be more meaningful than only presenting a logistic regression of some form. A more reasonable approach (or at least equally

justified) to analyze the data would be a multi-level logistic model (as effects can be assumed to be random, in addition to accounting for the nesting). Some parts of your writing suggest that you indeed tested this and that results favored the interaction to a lesser extent. Another common alternative would be an individual participant meta-analysis (given that you have all data at this level). This analysis would also provide important information, such as the heterogeneity of effects. This is important as you sometimes write about “significant heterogeneity” (l.234) but do not seem to test this directly. GAM is great too, and directly comparing it to the other approaches will be very valuable. Thus, I strongly advise you to present all these methods in a direct and well-documented comparison to drive home your argument of analytic flexibility leading to different results. Please use efficient visualization to compare the findings, as figures are currently underused in the manuscript.

It is not clear why not including an interaction in a model is the same as constraining it to zero: “we’ll use the ideology factor from the LRP studies and constrain the three-way ideology-personality-veracity interaction to zero by not including it in the regression model.” Usually, constraints to zero (for instance, in SEM) require an interaction to be explicitly modeled.

When an interaction is non-significant, the typical convention is not to follow up testing for marginal effects. In the case of the three-way interaction, you not only follow up on this effect but also reduce the variance through median splits, which is generally not advisable. If the interaction is non-significant, that suggests that the slopes do not differ significantly, even though one may have a p -value above and the other below a given threshold and, correspondingly, somewhat different effects. The new analytic comparisons suggested above will provide further insights into the robustness of the three-way interaction, but generally, conscientiousness simply seems to make partisans reluctant to share any type of information. This does not mean that low C individuals should by principle not be targeted by intervention, but it requires a thorough discussion of what unintended consequences this may have. This is especially important as it concerns a prominent debate in the field.

Relatedly, statements such as “In many situations, simulations and visualizations are superior tools for understanding the nature of an effect rather than focusing on a single coefficient” need to be thoroughly backed with evidence. Many scholars would disagree quite strongly with this take. For instance, what consequence does this approach have for generalization of results? It becomes even more puzzling why this argument is further made to explain inconsistent results in meta-analyses.

In slope graphs, 95% CIs need to be visualized.

I.205, the interpretation of main effects is complicated when interactions are included. They usually do not refer to typical main effects anymore.

Marginally significant effects need to be interpreted with caution and appropriately discussed in the Discussion section.

Table 3 is hard to understand. Are these zero-order correlations? Also, you need to number the variables for the numbers in the first row to make sense. The table also needs to present *p*-values in some form. Again, I think a meta-analysis will be better. For instance, the “ALL” column needs to weigh effects by sample size. Essentially, a meta-analytic approach.

In some places, you seem to suggest that a main effect of political orientation is needed to observe an interaction, but this is not the case. For instance, it is possible that opposite effects of political orientation on each side of the conscientiousness spectrum cancel each other out. You note this later in the manuscript, but this is an essential point also highlighted by one reviewer.

An argument is made about the time and stimuli. If indeed the veracity or other aspects of the stimuli changed, changes in ratings of these items should be predicted by the time the study was conducted. This could be directly tested (time as a moderator or main and interaction effects).

It is not clear why no *p*-values or 95% CIs are presented from bootstrapped correlations.

LINGUISTIC CHANGES

Avoid colloquial abbreviations such as “it’s,” “let’s,” “we’ll.”

Can you present the number of observations in addition to participant Ns in the abstract as well?

Please format tables thoroughly in APA7 format.

Make sure to capitalize headings in line with APA7.

I.288, it is unclear what is meant by “effects” being “developed.” I assume you mean “studies.”

Table 4 appears detached from the manuscript. It needs to be described in more detail in the main text.

To sum up, there are many strengths in your work, but the potential of this manuscript is not yet fully realized. I understand that these requested changes are comprehensive. Thus, although I would appreciate receiving a revised version by September 6, do not hesitate to reach out to me if you require an extension.

Best,

Jonas R. Kunst

Reviewer 1:

Thank you for carrying out this important work, and for giving me the opportunity to review its contents. I believe the aims and considerations addressed throughout are timely and vital to reconcile in the misinformation literature, and I think the authors do so relatively well.

The analysis of published work is comprehensive and clear, using impressive methods and considering its limitations and implications. The writing style is clear and concise, demonstrating detailed knowledge of the literature and more specific concepts dealt with in the manuscript. While I think this work would be an important and helpful contribution to the literature, there are a few concerns I would like to raise before it can be considered for publication.

The nature of the primary interaction effect observed

It's not quite clear to me what type of interaction was consistently uncovered between conscientiousness and ideology. From the majority of the plots, it seems like an attenuation interaction in which the distinction is larger among more conservative individuals is observed? If so, this would be quite helpful to spell out. On lines 319 to 322 you outline Lawson and Kakkar's (2021) prediction that this interaction effect will not be a reverse interaction, but this still doesn't clarify whether a knockout or attenuation interaction is expected. Regardless of the difference between hypotheses and findings, the types of interactions have important implications for powering studies. If researchers can be made aware of the type of interaction observed they are more informed about what to expect when calculating their a priori power analyses. Some more consideration of why we might expect the type of interaction we observed conceptually might also help for future research.

Why conscientiousness?

Throughout the manuscript, it's not quite clear why we should expect conscientiousness to play the role that it does, or indeed what to expect for the other Big Five variables. It may be clear from the literature cited, but I think more consideration needs to be given to the underlying processes of the Big Five traits that might make them interesting candidates for explaining the relationships investigated here. Of note, there are a few meta-analyses now that demonstrate how

none of the Big Five traits are consistently associated with beliefs in conspiracy theories, an outcome measure that could arguably be considered adjacent to misinformation susceptibility and sharing. Why might general misinformation sharing, but not conspiracy beliefs be related to these variables? This is more of a conceptual point, but I do believe elaborating and/or clarifying the nature of these relationships might strengthen the applicability of this important work.

What about real news?

In your paragraph featured from lines 470 to 488, you rightly argue for standardizing the presentation of different approaches to measuring sharing intentions/behaviours. However, this seems like a missed opportunity to also present real news sharing on its own, as you've done for fake news sharing. While I agree that your analyses of the proportionate relationships between real and fake news sharing suggest that the effects observed here are about fake news being shared more frequently relative to real news, it might be interesting to see whether real news sharing follows the same/a similar trend regardless.

Implications of study design for the real world

I believe some consideration to the implications of research designs and their comparison to real-world effects might be helpful for readers. For example, the majority of people simply do not share—or in fact, “like”—anything on social media. Does this have implications for your findings? What about the analyses? I think some recommendations for researchers on how to deal with these expected floor effects may be warranted. For example, should we use Poisson regression? Should we Bootstrap our analyses? You of course gave consideration to these points when discussing Bayes etc, but I think a bit more detail is required to make some really helpful recommendations for the future. Perhaps consideration of the distinction between intentions and real-world behaviours is also warranted.

Minor comment

I believe footnote 6 is redundant because the information is also included in the main text on lines 430 to 432.

Reviewer 2:

I enjoyed reading this manuscript about the conflicting results and debates in misinformation research and the examples the authors use to explain how methodological choices impact the link between ideology and the shaking of fake news via the moderating role of personality traits, such as conscientiousness.

I should say, for transparency, that I am familiar with this particular debate and have refereed work on both sides before so I do feel sufficiently informed to review this work but have no horse in the race myself.

Overall, I thought this was a very nuanced and open-minded take on the issue and I appreciated the attempt to draw out the larger implications for misinformation research using other relevant examples. Look, I think the point that is being made is

not whether the authors' exact estimation strategy is the correct one or not, but rather that clustering on study, participants, or both (all valid strategies), create results that are quite different from one another and what others have reported using a different estimation model. The same is true for the preferred measure of ideology that is used across studies. I think these points are worth making and informative for how we think about these issues in the field.

The authors mention the debate between Pennycook, Rand et al and Rathje, Van Bavel & van der Linden et al on the moderating role of ideology in explaining the effectiveness of accuracy nudges. Martel et al. (2024) did a multiverse analyses, where 50% of the analyses indeed showed lower efficacy for conservatives on the type of accuracy nudge pre-registered by the Rathje team but not consistently for others. I think it's indeed a similar issue of the results depending on the measure, type of intervention, and estimation method.

The authors mention a few times how the stimuli are all different over time and across studies which makes inter-study comparisons difficult. They may wish to mention the new Misinformation Susceptibility Test (MIST) introduced by Maertens et al. (2023) which provides the first standardized and psychometrically validated headline measure of news veracity that was introduced precisely to help solve this problem.

The authors mention Signal Detection Theory (SDT) and the results switching based on which type of analysis is applied. This is an interesting one as SDT depends on the outcome being binary (true vs false) but most misinformation is misleading in degrees not 100% true or false so the analytical approach should fit the theory/method/stimuli. Reasonable people can disagree of course, but I agree that encouraging research to pre-register their rationales in light of these issues is a good thing.

I really liked the authors' point about how veracity discernment interactions (a single coefficient) can be misleading and how a significant coefficient is not necessary to try to understand what goes on in the larger media ecosystem, i.e., plotting the sharing of both true and false news using distributions or simulations is more insightful than focusing on a discernment coefficient. I broadly agree that this prior recommendation needs adjusting and the authors approach is preferable in this regard.

The Table with recommendations for future research strikes me as helpful

In short, other than the minor notes above, I really don't have any major issues, great paper and commentary!

Round 2

Dear Authors,

Thank you for submitting your revised manuscript. Your attention to our comments has substantially improved the paper. Given how thoroughly you addressed them, and as I see few substantial issues that require external consultancy, I have decided

not to send the paper out for further review. However, I have a series of requests that need to be addressed before I can accept the paper for publication. Please address each of them thoroughly and outline the changes in a revision letter.

First off, I would like to suggest a change in title that reflects the comprehensive and detailed nature of the paper it has become. What do you think about "Resolving Conflicting Findings in Misinformation Research: A Methodological Perspective"?

Next, I appreciate the more organized analytical approach. However, given the high number of analyses, it is important to explain the flow of content to the reader early on in the "Analyses" subsection of the "Methods" section. Here, please walk the readers through the chronological order of analyses and the rationale behind each step. For instance, at the moment, a meta-analysis is suddenly presented in l. 183, but it is not clear how this was specified, how many levels it included, what estimation it used, and which program and libraries were utilized. In the "Analyses" section, also make sure to define terms such as "Specification curve analysis". The simulation also needs to be described here in detail (some of it is currently in a footnote).

"Risk tolerance" is not an ideology, and it makes little sense to treat it as such (even though the original authors may have done that). The same applies to trust. This is also supported by the factor analyses. I suggest dropping both variables and explaining why in text.

Please generally avoid footnotes for substantial content such as analyses. Generally, please keep footnotes to a small number (< 3), preferably none.

Please be careful in how you describe null effects. For instance, "There is related meta-analytic evidence suggesting that the Big Five are not associated with beliefs in conspiracy theories (e.g., Goreis & Voracek, 2019)." Unless this was a Bayesian meta-analysis, you should write "are not statistically significantly associated with" instead.

Linguistic and Formatting Issues

Please capitalize headings and format figures and tables (titles, captions, etc.) in line with APA7. It is essential to clarify in the captions of the tables what analyses were conducted, as it is easy to get lost given the comprehensive scope. Please also make sure to define all acronyms (e.g., "95% CI", "r", etc.), including the study

labeling in the notes of figures and tables. The idea is that these should be independently interpretable.

“CI95” should be “95% CI”.

I^2 and df need to be italicized in line with APA7.

This formulation is odd, please revise: “Gathering evidence on research questions requires a precise definition of what quantity is trying to be estimated.”

“...with our central contribution being that visualizations are a critical corollary of statistical models to ensure that effects are well understood.” I think it would be more accurate to write “simulation and visualization.” (this may apply other places too)

This formulation can be misunderstood: “Yet there was variation across studies, with this correlation varying between $r = -.01$ (Study S1) and $r = -.35$ (Study S2), though all correlations were significant at $p < .001$ except in Study S1 ($p = .276$).” I think you mean that all “meta-analytic correlations” were significant, not effects such as $r = -.01$. The same applied to I. 205.

Under “Conscientiousness as a direct predictor of sharing fake news” it is not clear why warmth toward Republicans suddenly is reported.

I. 286-287: “Across the entire 52,812 observations combined, the former had an internal reliability of .86, indicating acceptable 287 reliability, the latter had a reliability of .52, falling beneath traditional standards for reliability.” This does not seem accurate. The ideology measures are assessed at the participant level, not the item observation level.

Please provide the factor analytic results in the SOM. In text, specify what type of estimation (ML?) was used. Generally, any result not fully reported in the main text needs to be presented in the SOM file and referred to. For instance, “All five of the ideology measures displayed significant interaction effects between conscientiousness and the measure of ideology ($ps < .001$).” This result needs to be presented in detail in SOM and referred to in the main text.

There is a typo in footnote 5.

In the note to Figure 1, simply state that the error bars represent 95% CIs. The same applies to Figure 2.

This is important: Please use different colors for the datapoints in Figure 1 and a corresponding legend to indicate what study designs the effects belong to. The mono-colored graph is little informative at the moment even though this graph is a key part of your results.

Please make sure to present all figures as SVG files, directly exported from R and inserted into the word document.

There is a typo here: "...there are differences in how the data was produced"

You state, "There is little evidence for a substantial three-way ideology-conscientiousness-veracity interaction." This requires to be supported by references.

I thank you once more for addressing our previous issues in detail. The paper has come a long way in becoming a strong contribution to the literature. What is left are a series of mostly minor, presentational issues, and I would appreciate receiving a revised manuscript within 1 month (i.e., by Nov. 21), preferably faster to expedite the publication of this important paper.

Best,

Jonas R. Kunst

Editor-in-Chief